follow this subject out further now because it is part of a larger one, which deals with the atmospheres and densities of all the bodies of our system, and to discuss it at length would lead me too far from the present subject; suffice it to say that the enormous atmospheres of Saturn and Jupiter and the absence of a lunar atmosphere result from one single cause, that cause being if I mistake not the chemical constitution of the exterior parts of the solar nebula as each planet was thrown off, and the subsequent action on each globe.

To come to details. Whether the walled plains are really due to volcanic action or not, our authors offer no opinion beyond referring to the hypothesis of Prof. Dana, as being the most rational. Dana suggested that, as at Kilauea, the lava was simply boiling, and gradually extending its boundaries from a centre, so that at last, if the heat continued, a quasi-crater might be formed of any extent. That the smaller craters are true craters Messrs. Nasmyth and Carpenter take great pains to show, and their evidence is of the closest and most satisfactory kind. The woodcuts which we produce will show how the central cones, which *are scarcely ever absent in the craters we are now discussing*, have originated on the theory advanced; at the same time it is shown how, on this idea, even when the central cone is absent or the crater is filled to the brim, as in the case of Wargentin, one of the most curious of lunar objects, the effects observed may be explained.

Mr. Nasmyth long ago illustrated the bright streaks on the moon by the experiment which he here details, and a very striking one it is. He took a glass globe, filled it with water, and having hermetically sealed it, plunged it into warm water, "the enclosed water expanding at a greater rate than the glass, exerts a disruptive force on the interior surface of the latter, the consequence being that at the point of least resistance the globe is rent by a large number of cracks diverging in every direction from the focus of disruption."

From the photograph, it is clear that the result is strikingly similar to the rays which have Tycho for a focus, and on the strength of this similarity the authors claim this as another effect of expansion due to cooling. I so, however, the experiment with the glass globe is not in point. But however the cracks were produced, they imagine that thus having them travelling along for hundreds of miles irrespective of surface inequality, there was an up-flow of molten matter which spread out on both sides and turned the narrow crack into a broad streak.

I trust I have said enough about this book to induce all interested in physical problems to peruse it for themselves; it is altogether an admirable production, and if space permitted each picture even would merit a special paragraph.

J. NORMAN LOCKYER

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Natural Selection and Dysteleology

IN his reply to a criticism which appeared in NATURE, Prof. Struthers alluded to a question of considerable interest to evolutionists, viz., whether or not the presence of useless organs "proves too much for the argument."* The difficulty is one often met with, and has been well stated by Prof. Huxley, thus:—"Prof. Haeckel has invented a new and convenient name, 'Dysteleology,' for the study of the 'purposelessnesses' which are observable in living organisms—such as the multitudinous cases of rudimentary and apparently useless structures. I confess, however, that it has often appeared to me that the facts of dysteleology cut two ways. If we assume, as evolutionists in general do, that useless organs atrophy, such cases as the existence of lateral rudiments of toes, in the foot of a horse, place us in a dilemma. For, either these rudiments are of no use to the animal—in which case, considering that the horse has existed in its present form since the pliocene epoch, they surely ought to have disappeared—or they are of some use to the animal, in which case they are of no use as arguments against teleology," &c.†

Now it appears to me (as I think it must to all upon adequate consideration), that the dilemma thus presented is only apparent; its first-mentioned horn having no existence. In other words, we can never in any single case be sure that any length of time would have been sufficient to enable natural selection totally to obliterate a useless organ.

Mr. Darwin, in the "Origin" and in the "Variation," has mentioned three causes which operate towards the removal of useless structures. These causes are Selection, Disuse, and Economy of Growth. Recently he has suggested a fourth cause, which may be epitomised as the dwarfing influence of impoverished conditions, progressively reducing the average size of the useless structure, by means of free intercrossing.‡ I shall endeavour to show that these causes are not sufficient to obtain the complete extinction of rudimentary parts in all cases.

Selection may be considered as applying only to those rare instances in which changed conditions of life may be supposed to have rendered a previously useful organ injurious; for so far as selection operates in obliterating a merely useless organ, it will be more convenient, for the sake of brevity, to identify it with the Economy of Growth.§ Since, however, it is obvious that an injurious organ must pass through the merely useless stage before it becomes wholly suppressed, we may dismiss this cause without further comment.

Disuse and Economy of Growth are so much entangled in their operation, that it is hopeless to find examples illustrating their separate action. In so far, therefore, as we choose to disentangle them, we must discuss the question in the abstract.

Disuse may be left out of the question, so far as its influence is due to the principle of inheritance at corresponding periods of life; for, as Mr. Darwin tersely observes, "as most organs could be of no use at an early embryonic period, they would not be affected by disuse; consequently they would be preserved at this stage of growth, and would remain as rudiments." ‖ It may,

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* Nature, vol. ix p. 83.

† "Critiques and Addresses," pp. 307–8.

‡ Nature, vol. viii. pp. 432 and 505.

§ I think, also, that if the Economy of Growth is really a principle independent of the "more general principle," viz., the *direct* influence of "natural selection in continually trying to economise every part of the organisation" (Compare "Origin," 1873, pp. 117–18), it may yet, without any great stretch of inference, be considered as due to the *indirect* influence of natural selection. For, the survival of the fittest demands that each individual shall utilise its stock of nutriment to the best advantage; and, this demand being continued through many generations, it does not seem in itself improbable, that it should thus at last secure to all the members of the surviving species an inherited tendency so to economise their nutritive power when fresh occasion requires—an advantageous innate *temperament* distinct from the external *eliminative* agency of Natural Selection. Only in some such way can we account for the facts of acclimatisation, in those cases where the adaptive changes take place immediately after the transportation of the organism: also for an analogous class of facts, such as that of the shells in the same species of Mollusca, differing in their thickness upon the weather and the lee sides of the Plymouth breakwater.

‖ "Variation," vol. ii. p. 317. I may mention, also in passing, that it seems to me not unopen to question whether disuse is the *principal* cause even of reduction. There is no doubt that disuse causes more or less of atrophy in the individual, and from this fact it is argued, that the principle of inheritance at corresponding periods of life must entail the continued reduction of a disused part in the species. Now the only effect of the principle relied on is that of prolonging, as it were, the life of the disused part over many generations—thus affording it an indefinite time to succumb to the conditions which reduced it in the life of the individual. But it is necessary for the validity of the inference that it would so succumb, to show that these conditions are the efficient causes of this reducing process in the one case, as they prove themselves to be in the other. Suppose, for instance, that failure of nutrition is the principal cause of atrophy under disuse, does it follow when such failure has done, to all appearance, its utmost during the life of the individual (as we see in disease), that it could do any more were that life indefinitely prolonged? Of course in the case of short-lived animals, where the dwarfing influences may not have time to exhaust themselves in a single generation, the principle of inheritance at corresponding ages may be drawn

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however, be objected that the doctrine which Mr. Darwin elsewhere inculcates,[*] and deems sufficient to account for the total suppression of rudimentary organs, viz., inheritance at earlier periods of life, is fatal to dysteleology as a prop to evolution—at least in the case of long-lived species. And so it would be, were not this principle of so shadowy an application that, while it is perfectly legitimate to point to it as a possible cause of total suppression in some cases, it would be simply absurd to argue that such must be its effect in all.

We next come to the Economy of Growth. Suppose an organ to become suddenly useless, this principle would at first cause its rapid reduction. In proportion, however, as its presence ceases to be injurious, the arresting process becomes slower and slower, until a point is reached at which it is presumably *nil.* That such a point of rest must somewhere be attained seems evident, if we consider that the smaller the diminishing organ becomes the less is it subject to the influence of the Economy of Growth. In other words, when the organ undergoing reduction becomes so minute relatively to the size of the animal (or, more correctly, to the available store of nutrition), that the supply of nourishment it requires is no longer perceived by the organism at large, it then remains permanently of that size. "The economy of growth will not account for the complete or almost complete obliteration of, for instance, a minute papilla of cellular tissue representing a pistil, or of a microscopically minute nodule of bone representing a tooth;"[†] and, the whole principle being one of relation, it is a question, for instance, whether the rudimentary digits of a horse consume a greater relative amount of nutrition than does the "minute papilla." Besides, without entering into details, I think there is very good reason to believe that the Economy of Growth is unable to reduce an organ which was originally large, to the same absolute size as it can an organ which was originally small. From all this it follows, that if the struggle for existence were in any case so keen as to afford Selection (*i.e.*, Economy) the opportunity of totally obliterating every rudimentary organ, it seems probable that the species itself would require to become extinct.

Turning now to the last of the causes propounded by Mr. Darwin, there can be no doubt that it is (theoretically) sufficient to procure total obliteration. Forasmuch, however, as we can never know in any given case whether or not the requisite conditions have been supplied,—*i.e.* impoverished nutriment for an enormous length of time,—this newly added cause affords no further justification for the old statement, that the theory of Natural Selection fails to account for all the facts of Dysteleology.

The perusal of the last-mentioned thoughtful conception has suggested to me the probable existence of another cause, having a more general application; but as it can never induce complete suppression, I shall reserve it for the subject of another communication.

Mr. Mivart supposes that organs may become suddenly aborted[‡]; but, apart from the weighty objections to this view,[§] there is no case on record, so far as I am aware, of an organ thus becoming *totally* suppressed in any domestic species. A sport of this kind always leaves a rudiment, and it is upon the analogy of such sports alone that Mr. Mivart's argument is founded.

Having now enumerated all the causes ever proposed by evolutionists to account for the reduction of useless parts, it is evident that we should antecedently expect to find innumerable examples of such parts in the condition of rudiments.[||] Indeed the only difficulty is to account for that final disappearance of organs which must, by any theory of evolution, be postulated to have taken place. The solution is afforded by the exhaustive contemplations of Mr. Darwin, for, whether or not we believe in pangenesis, we cannot but deem it in the highest degree probable that the influence of inheritance is not of unlimited duration. If so, we have at once an adequate cause for the eventual destruction, even in the embryo, of rudimentary parts; but, as it is a cause which would only act after an immense lapse of time, it would have no influence until the original specific type had undergone a considerable modification. Thus, the facts of dysteleology, far from "cutting two ways," afford the strongest confirmation of the natural selection theory; for, as time is thus shown the chief agent in the final destruction of rudiments, and as species are always undergoing change, on the one hand we have an explanation of the fact, that the greater the divergence of the specific type from its original the fewer rudiments do we find of organs characteristic of the latter, while on the other hand, the less such divergence the greater the number of such rudiments—a fact of which the necessary consequence is, that "with species in a state of nature, rudimentary organs are so extremely common that scarcely one can be named which is wholly free from a blemish of this nature."

GEORGE J. ROMANES

The Action of the Heart

HAVING replied to Mr. Garrod's criticism of my "Locomotion of Animals" (NATURE, vol. ix. p. 281), I now wish to show that the explanation given by him of the diastole of the heart is not in accordance with fact.

In a recent number of NATURE (vol. ix. p. 282) I asked Mr. Garrod to explain "how the left ventricle of the heart opens after a vigorous contraction in which all the blood contained in the ventricular cavity is ejected and the ventricle converted into a solid muscular mass, if not by a spontaneous *elongation* of all its fibres." He replies:—"At first sight it might seem that the active dilatation of the heart during the diastole did depend on an inherent power in the muscular fibres of the ventricles to elongate, but the peculiarities of the coronary circulation are quite sufficient to explain the phenomenon without the introduction of so unnecessary a theory as that of Dr. Pettigrew. For in the heart when removed from the body, as in the living body during diastole, the injection of fluid into the coronary vessels causes the whole heart to open up from the congestion of the ventricular walls, and so produce the active dilatation which is well known to occur" (NATURE, vol. ix. p. 301).

The explanation given by Mr. Garrod of the manner in which the ventricles of the heart open up during the diastole is eminently unsatisfactory. In fact it is no explanation at all. He informs us that the active dilatation of the ventricles is due "*to peculiarities in the coronary circulation*" . . . "for in the heart when removed from the body the injection of fluid into the coronary vessels causes the whole heart to open up from the congestion of the ventricular walls, and so produce the active dilatation which is well known to occur."

The coronary vessels, as everyone is aware, simply supply the blood which nourishes the substance of the heart. There is no peculiarity whatever in the circulation of the blood through them. The blood flows through the coronary vessels in a more or less steady stream as through all the other vessels of the body. In other words the substance of the heart is full of blood during the closure or systole of the ventricles, as well as during the opening or diastole of the ventricles. The presence of the blood, therefore, within the coronary vessels can exert no influence in opening up or actively dilating the ventricles. This is proved by direct experiment. If the heart be cut out of the living body and the coronary vessels divided, the ventricles go on opening and closing with the utmost regularity for protracted periods. Here, however, the power which, according to Mr. Garrod, opens the ventricles is inoperative. The same thing happens when the heart is cut out of the body and the vessels laid open. If, however, the ventricles open and close when the coronary vessels are freely divided, and the blood which is said to distend or open up the ventricles is allowed to escape from the cut surfaces, it is quite clear that the blood pressure within the ventricular walls can exert no influence whatever in producing the diastole. If blood is not admitted into the coronary vessels, or if admitted it is allowed freely to escape, it cannot of course act as a distending medium. Allowing, however, for the sake of argument, that the flow of blood through the vessels of the ventricles occasioned the opening or dilatation of the ventricles, it is evident, for the same reason, that the presence of the blood within the ventricular walls, from the fact that the blood is nearly constant in quantity and virtually incompressible, would prevent the closing or contraction of the ventricles. Mr. Garrod, I opine, is

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upon until the point of such exhaustion is attained; but is it not open to question whether this point can never be reached at all? It must de remembered, too that in the case of wild species the effects of disease are always associated with other reducing causes, so that here we may easily over-rate the share it has in the work; but in the case of domesticated species the effects of disease are much more isolated (in consequence of Economy of Growth, etc., being, to a great extent, in abeyance); and here we find that atrophy of disused parts, although at first very rapid, eventually does not proceed to nearly so great an extent as it does in the case of wild species. The question thus raised, however, is of no practical importance, since whether or not disuse is the chief *cause* of atrophy in species, there is no doubt that atrophy *accompanies* disuse.

[*] "Variation," vol. ii. p. 80.

[†] "Variation," vol. ii. p. 397.

[‡] "Genesis of Species," 1st ed. p. 103.

[§] See "Origin," pp. 201—204.

[||] It is unnecessary to consider the *collective* action of these causes, for a moment's reflection will now make it evident that none such exists below the point at which the Economy of Growth ceases to be felt.

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